

THE
GOULSTONIAN LECTURES
ON
PUERPERAL FEVER.

Delivered at the Royal College of Physicians, London.

BY
ROBERT J. LEE, M.D., F.R.C.P.,

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CURATOR OF THE MUSEUM, ST. GEORGE'S HOSPITAL; LATE PHYSICIAN TO THE
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FORENSIC MEDICINE AT THE WESTMINSTER HOSPITAL;
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THE GOULSTONIAN LECTURES ON PUERPERAL FEVER.

LECTURE I.

MR. PRESIDENT,—The disease generally known as puerperal fever is distinguished by characters which render it peculiarly suitable as a subject for the consideration of such an audience as I have the honour of addressing on the present occasion.

We can trace its history in the literature of medical science more or less continuously from the present time to periods so remote, that we possess now only scanty records of them. In the progressive stages of historical inquiry, we find that puerperal fever, though comparatively a modern introduction into nosological classification, has retained a degree of exactness in its nature which can be claimed by few other diseases, which enables us to recognise it without difficulty in the descriptive works of various authors. It has shared the same fate—or perhaps we ought to say, has enjoyed the same privilege—as those to which I refer, in that it has afforded the student of history the means of determining the character of mind, the method of investigation, the degree of accuracy in observation, and the practical skill which were possessed and exhibited at any particular point of time within the compass of historical records.

Lastly, it may be regarded as the most recent and not least grateful recipient of the favour and advantage which every branch of our science and practice derives from the progress of physical knowledge.

If puerperal fever be thus distinguished, it would seem an easy task to entertain you without fatigue in any one of the lines of research which I have indicated. I am obliged, however, to request that the standard which I regard with deep respect, and you with satisfaction, may be reduced to meet an unusual demand for judgment and caution in dealing with materials so abundantly afforded by past history and

recent important researches as to make the satisfactory treatment of them a matter of more than common difficulty.

There is a particular mental attitude, if I may use the expression, which we must assume, in order to arrive at just conclusions regarding the value of the contributions of others to our stores of knowledge of any particular branch of science. It is not easy to avoid a tendency to become partisans of a theory under the influence of evidence which rests upon data beyond our immediate power of verification. To observe and record with accuracy the phenomena of disease, both clinical and pathological, requires much more than ordinary caution and patience when those observations are made for the express purpose of testing the credibility of the opinions of others.

It is desirable to keep in view the general principle that those facts are most valuable which contribute to the practical treatment of any malady, including of course in the term treatment the knowledge of etiology and the best means of prevention. It may be necessary to mention some of the various methods of treatment which have been at times in general repute, though with few exceptions I shall omit those which were of empirical character and ephemeral duration, and cannot be classed with remedies rationally suggested by etiological and pathological considerations, and fairly tested by experience.

It shall be my aim, therefore, when perusing the history of this fever, to prefer those records which contain practical information, and which tend to the elucidation of the origin and real nature of the disease. And, while pursuing this plan, we may with advantage notice instances in which contemporary neglect apparently rewarded most important researches. The effect of thus ascertaining the reception which was accorded to new and original views will be to render us less liable to the disparaging charges which posterity may make against us when present time has become the past—a time which, there are good reasons for believing, will prove not the least distinguished epoch in the future history of the progress of medical science.

I shall attempt to place before you such evidence as is necessary to show that puerperal fever existed very much in the same form as in the present day, at that period which we generally term “classical”, when nosological distinctions were based upon symptoms, and the modern science of pathology had not been developed, or was entirely unknown.

I may assume, for the sake of convenience, the date of the foundation of this college (A.D. 1518) as the commencement of the modern era, through which we shall trace the successive additions which were made to the general and special knowledge of our subject.

Within the last few years, very considerable modifications have been brought about in our views of contagious agents, in which of course those on puerperal fever have to some extent participated. It is to the chemical investigator and the scientific physiologist that we are chiefly indebted for a series of observations and suggestions which have thrown more light upon the real nature of the fever than we should ever have obtained from ordinary clinical and pathological methods. In fact, it may be said that those sources of information were exhausted. As the conclusions deduced from the observations alluded to are of great practical value, and are receiving constant additions now that so many are engaged in research, it will be necessary for us to examine with patient care the facts presented by the most reliable authorities, and endeavour to ascertain whether they will bear the test of general experience, and how far they afford satisfactory explanations of the causes which produce the phenomena which characterise the disease.

There is one point which must be noticed when we compare in detail the descriptions which we possess of the fever as given by some authors, with the general symptoms which it usually presents in the routine of daily practice. It is the fact that the disease has often appeared in a more acute form, and has been attended by more serious results, than we observe now. Those epidemics, as they were called, which occurred in some of the large cities of Europe and in this country, particularly in the middle and latter part of last century, afford us considerable assistance in explaining these apparent discrepancies, though a complete solution is only to be supplied by an accurate determination of the etiology of the fever. It may seem paradoxical to assert that the disease is a definite one, and yet that we are to be prepared to meet with various descriptions by authors of its symptomatic and pathological characters. If it were not for the fact that many have formed their own ideas of its nature from personal experience alone, and have obtained only a limited view of the many phenomena which it exhibits, it would have been unnecessary for me to direct attention to the reason why we should make some allowance for the difference in details and in opinions which I allude to. There is one way in which we may yield in this respect without going too far, and that is by separating the points of resemblance from those of difference, under the guidance of experience; and by not insisting too strongly on the introduction of the latter into the terms of a definition, we may agree that, both in a scientific and practical point of view, there is sufficient positive evidence to justify us in regarding the disease as one of specific nature.

The Hippocratic treatise known as the *Περὶ Γυναικείων* contains a general description of puerperal fever, which has been quoted by most writers on the subject. We may supplement it by references to the *De Morbis Vulgaribus*, or *Epidemics*, in which a few cases are related with such clearness and detail as to afford the evidence we require to prove the point in question.

Hippocrates says : “ If the lochia do not occur in the puerperal condition, it is very probable that fever and abdominal swelling will supervene. The whole system, but particularly the abdomen, will be highly sensitive to touch. Sometimes there will be intense abdominal and lumbar pain, dislike of food, insomnolence, and irritation of the surface of the body. On the fifth or seventh day after these symptoms, the patient will be attacked with disturbance of the alvine and urinary secretions. If the lochial discharge return, the patient may with proper care soon recover ; but, if not, there is great danger that diarrhœa will ensue, and the lochia be arrested. The arterial pulse is weak, though sometimes quick and varying in force. Thus the early stage of the disease is characterised. After a time, the hollows of the face become flushed. In this condition, the diet must be light. If there be delirium, laxatives must be given to drink, and other remedies, according to the indications of bilious or mucous secretions. Fomentations and poultices are to be applied daily. If, however, the lochial purgation appear to affect the head, thorax, and lungs, for that may happen, the patient always very soon succumbs.” (*Hippocratis Opera*, Ed. Foes., p. 603, secs. 40, *et seq.*)

This general description applies, as we see, to one particular form of puerperal fever, and, though it is followed by an account of some other forms of disease which we know to be nearly allied to it, and to depend upon the same cause ; yet there is no strong indication that they were associated for any reason of that kind in the treatise referred to.

To illustrate the above, in the first book of the *Epidemics*, the history is related of the case of the wife of Philinus, who was seized on the fourteenth day after delivery with pain in the hypogastric and pubic regions, consequent on the cessation of the lochia. The first symptom mentioned is rigor, or in the original : “ She was seized with fever attended with rigors.” On the eighth day after this, the rigor returned ; on the fourteenth day, she had subsultus and incoherent wanderings, and on the twentieth day she died. (*Op. cit.*, p. 976.)

This case is followed by one still more accurately described. “ The

wife of Dromeades, having been delivered of a female child, and all other matters progressing favourably, on the second day after was seized with rigor and acute fever. She began to have pain in the hypochondrium and nausea; was incoherent and sleepless for many hours; respiration was long and irregular, and she suffered from thirst, cold sweats, and epistaxis. On the sixth day, she expired." The third case is that of a woman who "was seized with fever soon after the birth of a son. At first, there was thirst, nausea, and cardialgia; the tongue was dry, the bowels disordered; she had slight rigor, acute fever, and a faint cold sweat about the head. On the third day, she was in great pain, and there was diarrhoea; on the fourth day, she had rigors again, and all the symptoms were increased in severity. They continued with slight variation till she died with epistaxis on the fourteenth day." (*Op. cit.*, p. 987.)

Some remarks on the conditions of the atmosphere which existed at the time of the occurrence of this case contain the observation that erysipelas, in a malignant and non-malignant form, was prevalent. There is generally some reason for the introduction of such a fact as this, which is left to the reader to explain as he pleases. The way in which Hippocrates quietly mentions it without remark, still leaves the impression that he had noticed a connection between atmospheric conditions and the occurrence of certain diseases which he could not explain.

There is another important case, which is compared by the commentators with that of the wife of Philinus. The symptoms are regarded as very typical in both instances of the consequences of arrested lochia: a theory, I need hardly mention, which satisfied the minds of the physicians of that day, and indeed for centuries later.

But I do not mention this case, which commences ἐν Θάσπῃ τὴν κατακειμένην, etc. (*op. cit.*, p. 1096), for any other reason beyond the fact that, during the period of eighty days of illness terminating in death, we have those symptoms detailed which clearly mark the case as one of phlebitis or inflammation of the cellular tissue of the right limb. The woman was attacked on the third day after delivery with the usual symptoms of puerperal fever, such as were mentioned in the other cases I have related. On or a little after the twentieth day, the symptoms of phlebitis commenced, and the febrile variations which are so commonly met with in this form of disease are simply and clearly stated; they were of a wandering and paroxysmal character (πεπλανημένως δὲ παροξύνομενοι), which exactly describes them.

Before disposing of the Hippocratic works, it may be allowable to refer briefly to two cases of abortion (*op. cit.*, pp. 1078-79)—cases, at least, in which febrile symptoms followed immediately on that occurrence. It is right to mention that they are referred to in the annotations of the edition of Foesius as instances of fever producing abortion. There is, however, this peculiarity in the account of the cases which we can hardly regard as without intention. It is, that they both commence somewhat thus :—"This is a case in which acute fever attacked a woman immediately after abortion," etc.; or, "This is another case in which acute fever followed abortion at the fifth month," etc.; and both cases terminated fatally on the seventh day. As it is not the purpose of Hippocrates, in his clinical histories, to connect cause and effect, but to leave the simple facts to speak for themselves, we are not obliged to submit to the conclusions of commentators who have looked at them through the medium of a theory. We are told of the events and symptoms in order of time, and we are permitted to consider the occurrence of abortion related in those cases as certainly presenting a different aspect in connection with the symptoms than if it had been mentioned later in the history.

Independently of these cases, we have the distinct statement, that abortion might be followed by puerperal fever; for we find, in the treatise *De Morbis Mulierum*, such remarks as the following :—"This (puerperal fever) may happen after abortion"; or, again, "Abortions are more dangerous than ordinary parturition, for the uterus is liable to ulceration or inflammation—a condition most serious". It is from such extracts as these and others that most modern authors satisfy themselves of the existence of puerperal fever among the maladies of ancient times. In a treatise by Hulme in 1772, the point seems to have been settled in the opinion of the authors of last century, and his concluding remarks are interesting enough to be quoted as the general expression of opinion at the time referred to. "We have here", he says, "a pretty accurate and plain description of the puerperal fever, as observed by Hippocrates, which in most parts exactly corresponds with what has been said of it in the first part of this treatise, insomuch that, had both descriptions been written about the same time, it might have been disputed which author had borrowed from the other. But the truth is, neither of them in the least tittle copied from the other, but from Nature alone. Hence it is similarity arises; from which also we may learn that the operations of Nature are the same in Britain as in Greece, and continue the same in this day as

they were above two thousand years ago. This is likewise a clear proof of the immutability of the puerperal fever, that it is an original disease, and hath been prevalent at all times and in all climates, and yielded to the same method of cure as hath been prescribed above." (Page 95.)

We may assume, then, that there is no great difficulty in establishing the first question of identity in all important particulars. There are one or two points deserving of further notice. It may be observed that Hippocrates omits all mention of treatment in his clinical examples, so that we are unable to determine the extent to which he practised the instructions contained in his general treatise, nor do we feel at all certain that we understand the views which were taken of the essential nature of this fever. It is asserted by his commentators that Hippocrates referred puerperal fever to an arrest of the lochia, the grounds for that assertion being his statement that this is the first symptom of the disease in the puerperal state. There is no doubt that he did entertain some such idea; for he remarks that, "if they"—that is, the lochia—"return, it is a favourable symptom". But the question of the cause on which the local and general disturbances depend is not limited by any means, as some suppose, to the arrest of the lochia alone. There are distinct pathological conditions assigned as causes in a certain number of cases; namely, inflammation and ulceration of the uterus, and retention of placental structures. There is no point more carefully set forth than this, and there is no hesitation in referring the symptoms which follow such retention to the effects of decomposition. "There may be ulcerations in the uterus from the decomposition of the lochia (Γίνεται δὲ καὶ ἑλκεα ἐν τῇσι μήτρησιν οἷα τῶν λοχείων σαπέντων)" certainly implies this. The word here used we have found necessary to introduce into our own language when we wish to indicate in a special manner the process of decomposition, though it appears to have been commonly used by the Greeks: as, for example, when Hermes reported of Hector's corpse:

δωδεκάτῃ δὲ οἱ ἦώς
Κείμενος οὐδέ τι οἱ χρώς σήπεται, οὐδέ μιν εὐλαί
Ἔσθουσι.

"This the twelfth evening since he rested there,
Untouched by worms, untainted by the air."

(*Iliad*, xxiv, 414, Pope's Translation.)

We shall see that the principle involved in our modern term "septic", though clearly appreciated by the ancients, was to some extent lost sight of till almost the present day.

So far as the arrest of the lochia was considered one principal cause of puerperal fever, the theory was naturally applied to the explanation of the various constitutional symptoms noticed in the disease ; that is to say, they were simply regarded as of metastatic origin, and their cessation on the return of the lochia was explained by and supported that idea.

Only one other circumstance remains to be mentioned under which the symptoms of puerperal fever may occur, and that is the possibility of inflammation at the ordinary period of menstruation. From what has been extracted thus briefly from the Hippocratic works, we may conclude that several causes were recognised by Hippocrates as capable of producing the series of symptoms which we term puerperal fever, and that amongst those there is evidently a clearly conceived notion of infective agency.

It is not probable, it is quite certain, that the more carefully we scrutinise the classic authors, the more they appear to have known of the symptoms of disease ; and their accuracy and acuteness of observation, and, above all, their singular simplicity and truth, command our respect and attention. It is only by some such general expression of appreciation that we can fairly excuse ourselves from a more elaborate analysis than I have offered above. The same kind of sensation steals over the mind in perusing those authors which the works of art of that period produce upon the senses. They exhibit a subtleness of intellect and perception which so astonish and delight, as to make comparison with contemporary efforts never occur to our thoughts, and which lull all feelings of envy into tranquil admiration.

It is not unprofitable, you will agree, to cherish the feeling I have described. It seems long since the Hippocratic writings were seriously studied in this country. We have perhaps been advancing, or at least moving so rapidly, that we have not cared to look back. Yet there never was a time, I believe, since the period of Grecian excellence, when such true appreciation of its wonderful productions would be developed by a more intimate acquaintance with them than at the present day. Though the knowledge of the phenomena of disease may never have been so complete, their origin and cause so profoundly investigated, as now ; though we have the assistance of mechanical, optical, and chemical science to aid in a thousand ways, and may be too much occupied with the present to think of the past—yet we may learn much by studying the too few relics we possess of ancient wisdom and knowledge, as in an equal degree we may acquire taste by

contemplating those ruined monuments—silent proofs of genius and skill.

In stating that we are in a position to judge of their merits, I mean to imply that there does not appear to have been any fair critical power exhibited even by the commentators of comparatively modern time. The annotations in the edition by Foesius of Hippocrates's works published in 1621, enable us to ascertain the exact state of knowledge during the long interval; and I may be excused for omitting to make any mention of a period in which science suffered at the hands of her enemies, and was adorned, consistently with Gothic feelings and ideas, in most unbecoming and uncomfortably restricted garments.

From the quotation in Dr. Hulme's treatise, and from those cases to which he omits to refer, it is clear that, so far as symptoms are concerned, all the important phases of puerperal fever were so well described by Hippocrates as to admit of no considerable addition or improvement. It is to the progress of morbid anatomy and pathology that we are chiefly indebted for recent information on the value of symptoms, and their relation to, or origin in, pathological processes. As examples of this, we are able to explain the various local and constitutional symptoms produced by those particular forms of the disease and their occasional complications, which we are accustomed to designate by distinct terms, *e.g.*, phlegmasia dolens, phlebitis, metritis, thrombosis, and others. When I said that we might assume the date of the institution of this College as an approximately correct and convenient starting-point of the modern era, it was intended to fix the attention upon one or two illustrious names which will always in this country be associated with anatomical and physiological science, and which retain their influence in the preservation of our national character of scientific thought and practical purpose.

For the present, we may limit our inquiries to the gradual evolution of the theory now daily gaining ground, that there is a particular cause for the various forms of puerperal fever; viz., the existence of a specific poison in the human system, either introduced or self-developed. It matters not for our purpose what the nature of the evidence was which first led to this belief, provided only that, so far as they went, the data were strictly true and the deductions logical. Our knowledge of the general principle, that we are all more or less under the influence of theories in our practical views of disease, enables us to discern to some extent from the method of treatment the particular theory which any individual maintains on any particular subject, without knowing directly

from other sources what that theory may be. Supposing that we have to determine the view entertained by some author of reputation on the nature of puerperal fever, we may ascertain from his directions as to treatment what his theoretical views were, whether chemical, mechanical, physiological, or pathological. This is not perhaps true of most of us, as we rather follow example than theory, and are more under the influence of education than those to whom we are indebted for original ideas. But, as we find there is often antagonism in the mind between the suggestions of a theory and the influence of habit, it becomes necessary to apply the test of treatment to determine how far any individual believes in his theory. In this question of the origin of the belief that puerperal fever depends on a specific poison, we find that the idea is by no means so recent as might be imagined; but, as it was regarded in such a doubtful way as not to influence treatment to any beneficial extent, we cannot admit that it deserves such serious notice as when we meet with those who have entertained the belief so strongly as to have used every endeavour in their power to arrest the influence of the poison, and to prevent its contagious effects. This is the reason for the above remarks. Time has proved that many explanations of the cause of puerperal fever have been erroneous, and that the belief in a specific poison is entitled to general acceptance. Now, if we limit ourselves to tracing its development, which has been sufficiently gradual, we shall be better occupied than in the criticism of erroneous doctrine, and we can perceive that, with such a limited object in view, we may unravel the tangles of history, and, by following one thread, do better than by attempting to begin without an end in hand on this or that knotted loop of conjecture.

In its present form, our idea of the specific nature of the poison which produces puerperal fever is somewhat different from that which was involved in the “*σῆψις*” of the Greeks; but to what extent it differs, and why, depends of course on our knowledge of the properties of the poison, and that point we need not consider at present. We are not obliged to meet the request to define what we mean by contagious; we should not allow an argument on the meaning of words to interrupt our attention to facts.

I now propose to examine how far the contagious theory of puerperal fever is supported, or contradicted, or neglected by the authorities of the seventeenth century. The theory of a poison generated in the process of the decomposition of organic matter involves the assumption that, *pari passu* with the process of decomposition, there is a

simultaneous and equivalent one of generation. This, perhaps, may be going a little too far ; but there is no doubt that decomposition and disintegration have always been clearly distinguished. To those who do not reflect upon such questions, the contrast would not seem so striking as to the mind of the scientific inquirer. It is the one, however, which called forth such a request as the following from the pen of Harvey, whom we may regard as the exponent of the most advanced opinions of his day.

“ What shall we say of the animalcules which are engendered in our bodies, and which no one doubts are ruled and made to vegetate by a peculiar vital principle? Of this kind are lumbrici, ascarides, lice, mites, syrcones, acari,” etc.; and then quoting from Aristotle : “ For in almost all dry things growing moist, or moist things becoming dry, an animal is engendered.” (Page 282, Trans. Harvey: *On Generation* (Sydenham Society's Works.)

In another place, he exclaims : “ Let physicians, therefore, cease to wonder at what always excites their astonishment ; namely, the manner in which epidemic, contagious, and pestilential diseases scatter their seeds, and are propagated to a distance through the air, or by some *fomes* producing diseases like themselves, in bodies of a different nature, and, in a hidden fashion, silently multiplying themselves by a kind of generation, until they become so fatal, and, with the permission of the Deity, spread destruction far and wide among man and beast” (p. 322, *op. cit.*) There can be no doubt of the views of the author of these passages regarding the development of minute independent organisms in the human body ; and, though we have made considerable progress in our powers of demonstrating forms of life of minute dimensions since his time, yet, for practical purposes, he had so clear an idea of the probability of the existence of almost atomic germs, that he would not only have believed the statements of Professor Tyndall, but have followed that distinguished experimentalist to the extreme limits of imagination. He would have agreed with the description of the poet :

“ Unfinished things one knows not what to call,
 Their generation's so equivocal :
 To tell them would an hundred tongues require,
 Or one vain wit's, that might an hundred tire.”

Essay on Criticism, part i.

There is all the proof which we require of Harvey's belief in the effects of a poison generated by decomposition to be found in a subsequent passage in his work on *Generation* ; and, as it is an important one in many ways, particularly in a practical point of view, I shall with your permission quote it at length.

“Women, as they alone have a menstruous, so have they alone a lochial, discharge ; added to which, they are exposed to disorders and perils immediately after birth, either from the uterus, through feebleness contracting too soon, or from the lochia becoming vitiated or suppressed. For it often happens, especially in delicate women, that foul and putrid lochia set up fevers and other violent symptoms ; because the uterus, torn and injured by the separation of the placenta, especially if any violence have been used, resembles a vast internal ulcer, and is cleansed and purified by the free discharge of the lochia. Therefore, do we conclude as to the favourable or unfavourable state of the puerperal woman from the character of these excretions.

“For, if any part of the placenta adhere to the uterus, the lochial discharge becomes foetid, green, and putrid ; and sometimes the powers of the uterus are so reduced, that gangrene is the result, and the woman is destroyed. If clots of blood, or any other foreign matter remain in the uterine cavity after delivery, the uterus does not retract nor close its orifice, but the cervix is found soft and open. This I ascertained in a woman, who, when labouring under a malignant fever, with great prostration of strength, miscarried of a foetus exhibiting no marks of decomposition, and who afterwards lay in an apparently dying state, with a pulse scarcely to be counted, and cold sweats. Finding the uterine orifice soft and open, and the lochia very offensive, I suspected that something was undergoing decomposition within. Whereupon I introduced the fingers, and brought away a mole. . . . The woman was immediately freed from her symptoms, and in a short time recovered.” (*Op. cit.*, p. 544.)

On the principle advocated by Harvey, that we do well to treat with respect the opinions of the past, I have made these extracts from his work on *Generation*. He observed that “respect for our predecessors and for antiquity at large inclines us to defend their conclusions to the extent that love of truth will allow. Nor do I think it becoming in us to neglect and make little of their labours and conclusions, who bore the torch that has lightened us to the shrine of philosophy.” (*Op. cit.*, p. 432.)

In order to ascertain how far the ideas of Harvey on the connection between puerperal fever and decomposition were entertained generally, we cannot refer to a better authority than the celebrated French obstetrician François Mauriceau. In his remarks on the suppression of the lochia, and the symptoms which are produced by it, he says that “those putrefying humours, by reason of their prolonged detention in

the cavity of the uterus, produce for certain considerable inflammation, so that the suppression of the lochia may be one of the most dangerous symptoms which can happen in the puerperal condition". (Ed. 1681, p. 284.)

The most important pathological effects of inflammation are described by Mauriceau, nor does he omit to mention the symptoms of puerperal phlebitis, attributing that also to the suppression of the lochia. This idea led him to depend upon bleeding as the chief remedy for the disease; so that we can hardly say he had any conception of the existence of a poison generated in the process of decomposition as the prime cause of these symptoms. As a clinical observer, however, he mentions in detail the most minute as well as the leading symptoms of nearly all the forms of puerperal fever with which we are acquainted; at least of those which occur in the puerperal condition, properly speaking. It is rather in his character of a practical physician that we must regard him than in that of a scientific pathologist.

I have already indicated to what extent simple clinical observation is able to explain the etiology of puerperal fever; and, for the reason that pathological analysis had not been brought to bear upon it till a century or more after Mauriceau's time, we have no important addition to notice in the works of intervening authors.

Changes had taken place, however, in the general doctrines of medical science during that period, and these necessarily more or less influenced general opinion on the etiology of our subject. I have used the term *pathology* in the sense in which we employ it at the present time, or at least in that sense which we attach to the term *morbid anatomy*. It had a different signification, however, a century ago, when Cullen said, in his lectures, "The pathology that I am to deliver is properly to be referred to two heads—that of causes and that of symptoms;" and again, "When we speak of the pathology of a disease in the institutions or in the practice of medicine, the disease is considered in its symptoms, causes, and effects." The term thus may be seen to have included the etiology as well as all the phenomena of disease.

As far as puerperal fever is concerned, we have only to notice in Cullen's works certain views which he entertained on the general question of the difference of fevers and their causes, and which were intended to apply to puerperal fever amongst others of contagious nature. It is his theory of contagion that we are induced to examine, and not any additions to the morbid anatomy or the symptoms of puerperal fever by

himself or his immediate predecessors, that deserve our attention. The term *fomes* was used by Harvey to describe a minute organic particle, one of the products of decomposition, possessing, very active properties, though certainly indefinite in its character. This term is also used by Cullen in much the same sense. He assumes the existence of different kinds of fomites, of different specific nature, and suggests the employment of the term effluvia "rather than the general ones of contagion and miasma". By effluvia he meant the products of the decomposition of any animal matter, as distinguished from contagion and miasma; miasma being the product of the decomposition of vegetable matter, and contagious being a term used to distinguish such diseases as scarlet fever, measles, and others, not originating in a decomposing process. Such I conceive to have been the meaning of effluvia, contagion, and miasma: "Substances", in his own words, "thus imbued with an active and infectious matter may be called fomites; and it appears to me probable that contagions, as they arise from fomites, are more powerful than as they arise immediately from the human body."

"The miasma", he says, "so universally the cause of fever is that which arises from marshes or moist ground acted upon by heat:" the word fever, as arising from miasmata, being restricted to the intermittent species. He defines "contagions as arising directly or originally from the body of a man under a particular disease, and exciting the same kind of disease in the body of the person to whom they are applied." (*Practice*, vol. i, p. 80, *et seq.*) From these passages, it is not difficult to understand Cullen's idea of fomites, though, as far as having proved the existence of such particles, he was not the least more advanced than his predecessors; that is to say, his reasons for believing in such germs of disease rested entirely upon clinical observation.

He found some difficulty, however, in dealing with a class of fevers or pyrexia known as "continued", jail, or hospital fevers, and in this class he included puerperal fever. He attributes their origin to a common cause. On this point, it will be seen that there was a definite opinion regarding the effects which may be produced by the effluvia from the living human body, which, "if long retained in the same place without being diffused in the atmosphere, acquire a singular virulence, and, in that state being applied to the bodies of men, become the cause of a fever which is highly contagious. The existence of such a cause is proved by the late observations on jail and hospital fevers, and that the same virulent matter may be produced in many other

places must be sufficiently obvious." With respect to these contagions, Cullen observes in another place: "Though we have spoken of them as a matter floating in the atmosphere, it is proper to observe that they are never found to act but when they are near to the sources from whence they arise; that is, either near to the bodies of men, from which they immediately issue, or near to some substances which, as having been near to the bodies of men, are imbued with their effluvia, and in which substances these effluvia are sometimes retained in an active state for a very long time."

With such views of the origin of what we now term septic causes of disease, we may expect that the treatment recommended by Cullen was consistent with those views. There is so little empiricism in his admirable works, and he always leads up to the subject of treatment in the most logical manner after a careful analysis of the symptoms and probable nature of any disease, that we are not surprised to find that he avoids those discussions which many of his contemporaries indulged in. It was not a question for him whether to bleed or not to bleed was right, nor would he have recognised any statistical contributions to the determination of that question as of much value. While as for special remedies, unless they were known by experience to produce such effects as would theoretically assist in the consistent working of his idea of the cause of the fever, would he have paid attention to them.

Cullen was not the man to be deceived by the representations of success which were reported to have followed the use of ipecacuanha in puerperal fever, in a memoir by Dr. Doulcet of Paris, the publication of which was made the year before Cullen's last edition was issued. His own directions for treatment, which we will notice presently, were based upon the view, "that the cause of death in fevers may be a poison; that is, a power capable of destroying the vital principle; and this poison may be either the miasma or contagion which was the remote cause of the fever, or it may be a putrid matter generated in the course of the fever. In both cases, the operation of such a power appears either as acting chiefly on the nervous system, inducing the symptoms of debility, or as acting on the fluids of the body, inducing a putrescent state in them." (Section 101, *op. cit.*) In other words, a "strong tendency to putrefaction in the fluids" was "the direct cause of all the phenomena of these fevers." (Section 102, *id.*)

Thus far I have advanced by rather rapid strides in the history of the most important part of our subject. I have referred to original sources for information, and have not attempted, under the influence of

any feeling, to make it appear that the authorities referred to knew more or less than they could fairly claim to have known. I have omitted almost all mention of a great deal that has been slowly dismissed from that total of data which we feel is more essential to correct conclusions than the assistance of the most powerful logical reasoning. We have arrived at a time when our modern system of medical science was beginning to develop itself; when attention was being directed to those new and unexplored regions which the researches of the morbid anatomist and pathologist have since exposed to our view. To what extent those new methods of inquiry modified the general ideas concerning the etiology of puerperal fever, I shall proceed to consider in the next lecture.

LECTURE II.

ONE fact, as Cullen's remarks have suggested, had gradually become evident in his day to those who were interested in the question of the cause of the disease ; it was the great mortality that characterised its occurrence in public institutions and hospitals. Long before 1788, when Tenon gave a report of the serious condition of the wards in the Hôtel Dieu, several epidemics, as they were termed, had been heard of. For more than a century before Cullen's *Practice* was published—that is, between 1652 and 1783—we have notices of this kind. Every author referred to them more or less directly when writing on the subject in the latter part of last century. One of the most distinguished observers of the disease almost commenced his treatise with the words: “The mortality attending the puerperal fever is truly lamentable. In the year 1750, at Paris, none who were seized with it recovered. In one hospital in London, in the space of two months, thirty-two patients were affected with that disease, and all except one fell victims to it.” (Gordon's treatise on the *Puerperal Fever*.)

This fact did not, however, appear in the same light to all. We have, among many other examples, that of an excellent physician, Dr. Hulme, whose work I have already mentioned, who makes the following remarks in the dedication of his treatise on puerperal fever to the Governors of the City of London Lying-in Hospital:—“Public hospitals”, he says, “for the reception of the sick and hurt are the grand seminaries of practical knowledge in the art of medicine. The utility of these institutions is so apparent, that they are now universally received all over Europe. Great Britain in particular hath not been behindhand in promoting such humane designs. Buildings of this kind, or which incidentally promote the same end, are to be seen in almost every part of this great metropolis. Among the rest, the City of London Lying-in Hospital rises up a simple, yet elegant monument of her beneficence.” There is a foot-note, to the effect that “this hospital is entirely supported by a voluntary annual subscription, whereby four or five hundred poor objects are admitted every year, and relieved with all necessaries during childbed”.

As Hulme was an authority on puerperal diseases, we may briefly notice that "the immediate cause" of the fever, in his opinion, was an inflammation of the intestines and omentum, and the "chief predisposing cause the pressure of the gravid uterus against the intestines and omentum". On similar principles of what we should hardly agree to call humanity, but with the additional object of teaching his pupils to "acquire true practical knowledge", Dr. Leake founded a hospital in Westminster, and Dr. Osborn and Dr. William Hunter both enjoyed opportunities of studying the disease in special hospitals.

We may conclude from this that, though Cullen's views were probably generally entertained, yet in the minds of many who were engaged in active professional duties, and who were certainly not deficient in intellect and benevolence, there was no distinct idea that the mortality in lying-in institutions might depend on causes over which there could be exercised very considerable control; so that the question of treatment appeared to be much more important than that of the etiology of the disease. I have mentioned one example of this in the recommendation of doses of ipecacuanha by Dr. Douleat. The report of the French Government on this plan concludes with the observation, that "the cure of a disease so active, and generally fatal in the Hôtel Dieu, and which allows so little time for reflection or hope from medicine, performed by a method so simple as that practised by M. Douleat, the success of which has proved so certain and uniform, is one of those extraordinary phenomena of nature which form an epoch in medicine".

We are not told exactly what other remedies M. Douleat employed; but, from the remark, "that those medicines most frequently administered in putrid diseases approach nearest to the method practised by M. Douleat", it would seem probable that his success may have depended on some other principle than that of promoting the action of the liver and intestines by emetics, which was one method, in his opinion, by which the effects of a poison might be eliminated.

As it is our object to trace the progress of exact knowledge rather than to review ephemeral views, we may content ourselves with the list of remedies which Dr. Meigs has introduced into his well known work as a quotation from M. Baudelocque, the younger of that name. This is the list:—"Blood-letting, emetics, purgatives, sudorifics, antiseptics, tonics, particularly camphor and bark; blisters and other revulsives, cold douches and cold stupes or hot stupes to the belly, subcarbonate of potash, oil of turpentine, mercurials: he ought to have added opium." Such were the differences of opinion on the subject of treatment.

As we have brought our review of the theories and different kinds of practice which prevailed at different periods, from the time of Hippocrates to the latter part of last century, when William Hunter, Cullen, Denman, and many other celebrated men, were contributing in their several departments to the advance of medical science in this country, I shall ask your careful attention to a work which apparently produced no great impression at the time it was published, but which has since had an important influence on our views of the nature of puerperal fever.

This work is by Dr. Alexander Gordon, and contains evidence that the fever depends on a particular poison, which may be communicated by direct contact from one person to another, and that the occurrence of a number of cases of the disease is to be explained in that way rather than by atmospheric or other conditions ; that is to say, that the disease does not occur epidemically. It is usual for modern authors to mention the names of Denman in England and Semmelweiss in Germany as the chief contributors to this important fact. When I say that these names are associated by modern authors, I mean simply that in our text-books we find some such sentence as the following :—"As a result of clinical observation, on the one hand, the febrile puerperal diseases were proved with certainty to be due to secondary infection first by the English (Denman), afterwards by the Germans (Semmelweiss)."

I am now quoting from the German text-book on *Pathology* by Klebs. It may be presumed that the priority thus acceded to us by German authorities has induced our own writers on the subject to be content with this division and distribution of fame. By way of diversion from the direct path of investigation we have been pursuing, I shall ask your attention to this question of priority ; not so much because it is of any great importance to determine how or when the fact was first brought to light that puerperal fever depends on a special poison, as to relieve the mind from an uncomfortable feeling that somebody may have suffered neglect. It always appears to me to be better to omit in educational text-books as much as possible all mention of names ; but, if they be mentioned, it is necessary that whatever is stated about them should be as certainly true as possible. It is a subject deserving of the consideration of the learned Fellows of this College, why there is such a scarcity of really good works for educational purposes at the present day. It will be answered probably, that science has made such rapid progress as to render it difficult to compose such works as text-books.

This is not the case in other sciences ; and I have not the slightest doubt that, if there were a general feeling of gratitude to those whose practical knowledge most fits them to undertake such difficult and thought-requiring labour, we should not be long before we could boast of possessing first-rate works of the kind I allude to. But to return to the question of Denman and Semmelweiss. Let us take Denman first.

It appears that we have more than one edition of his works, and that considerable periods intervened between their publication. In the last of these, there are the following remarks. "There is another consequence of an epidemic, or even a sporadic puerperal fever, on which it would be criminal to be silent. This is the contagious nature of these fevers, it having been long suspected, and being now fully proved, that they may be, and often have been, conveyed by midwives or nurses from one patient to another. This fact explains the reason why persons practising for many years with the most enviable success have at one or more periods of their lives, without any change in the principles or manner of their practice, met with a number of unfortunate cases when perhaps an adjoining neighbourhood has been entirely free from such diseases," etc. (Denman, p. 644, ed. 1805.)

There is no doubt about the meaning of this passage ; and, as it is contained only in the third and last edition, and not a word is said upon the subject of contagion in the former two, we may conclude that our German admirers have quoted from the most recent one. We can easily understand that such a general statement as "having been long suspected, and being now fully proved" is not quite satisfactory to their love of detail. However, Denman was quite right in saying so ; for, in the very year (1795) that he published his second edition, which preceded the third by ten years, the work of Dr. Gordon appeared, and, curiously enough, was dedicated to his intimate friend Dr. Denman. Now, Gordon's treatise contained the results of his observations on an outbreak of the fever in Aberdeen, which continued from 1789 to 1792, and then he allowed between two and three years to elapse before he wrote upon it. It is very probable that he communicated those results to Denman before the year 1795, when, as we have seen, his own work and Denman's second edition were published. If this were true, it would certainly lead us to conclude that Denman was not very willing to believe in Gordon's contagious theory when first proposed to him. If it be asked why did he not refer to Gordon's treatise in his last edition, we can only reply that, as he followed the example of the dog-

matic teachers of his day, he omitted on principle the mention of names, restricting himself closely to careful judgment of reported facts, applying to them the test of extensive personal experience. We may also conclude, from the circumstance that Denman was satisfied with the reasons adduced by Gordon for his contagious theory, that, in the interval of nine years between his second and third editions, that theory had been more or less generally accepted.

A very considerable change, in fact, had taken place in the state of knowledge of the nature and cause of puerperal fever. A careful analysis of the kind of evidence brought forward by Gordon to prove his point will show that it was much of the same character as that furnished by Semmelweiss. Neither of them used pathology or morbid anatomy to any great extent. The method they both employed was a species of close clinical observation, assisted by correctly logical deduction, and was rather of the nature of proof by circumstantial evidence than of actual demonstration.

It is impossible, however, on comparing the works of Gordon and Semmelweiss, to resist the conclusion that, although they were both possessed of powers of original thought and admirable truthfulness and courage, there was a considerable difference in their mental calibre. It requires a full appreciation of that peculiar instinct which is so rarely exhibited, and to which we owe all important advances in scientific knowledge, to estimate the great superiority of Gordon's intellectual and moral character. We can measure various kinds of force, but this mental force has never yet been properly valued. We cannot, of course, but admire the talent and industry which enable one man to work out such a problem as whether or not puerperal fever depends on a contagious poison ; but, when the idea is the suggestion of another, we cannot admit that he has any claims at all equal to those which we must accede to the one who first conceived the idea, and who, as far as he was able, applied experimental tests to it.

It is necessary, before we allow to Gordon the merit of superiority, to examine whether his theory was simply a conjecture, or well supported by observation. I should not have introduced this subject to your notice, if I had not felt pretty well assured that you would agree with me after such an examination. I had another reason : I wished to prove a fact of which we ought to feel certain as well as properly proud, and that is, that no country in the world can surpass ours in the production of men of that peculiar genius which in all departments of science characterises their chief pioneers.

Some recent authors have not been so generous to Denman as Klebs. The author of one text-book, which has been translated into English, gives to us the credit of ascertaining the fact, that puerperal fever is contagious or specific; but he claims for Semmelweiss the merit of having worked out the subject carefully, and of being essentially the author of all we know of the etiology of the fever (Schroeder). The growing tendency to copy such statements from foreign authorities into our own literature, many will agree with me, is not likely to encourage that kind of work and thought among us upon which the scientific merits of our profession depend.

Let us examine the data on which Gordon established the fact, that, to use his own words, "the cause of this disease was a specific contagion or infection", of which he asserted "he had unquestionable proof". He first undertakes to lay before us evidence that "every person who had been with a patient in the puerperal fever became charged with an air of infection, which was communicated to every pregnant woman who happened to come within its sphere". He submits to us a table of seventy cases, containing the name, age, and residence of each patient, and the name of the midwife or practitioner by whom she was delivered. Of course it is the latter column of this table which we are tacitly required to analyse. We are expected to notice certain names, either that of Dr. Gordon himself or those of midwives, in connection with several cases of the disease. Thus we find one midwife entered as having attended three cases on days closely following one another, and the same thing we observe to have happened to two others. It cannot be said that this table is altogether conclusive. It is not difficult to perceive that it only contains one part of the evidence which is necessary to make it so. At the time that these cases of puerperal fever occurred in a place like Aberdeen, it must have been, and was, remarked that there were other practitioners and midwives to whom such accidents did not occur, as Gordon mentions in his work; but it was impossible to reduce this important negative evidence into a tabular form. To make up for this deficiency, we have the evidence in another part of the treatise. As one instance, we may take the following:—"Now it may seem remarkable that the puerperal fever should prevail in the new town, and not in the old town, of Aberdeen, which is only a mile distant from the former; that it should prevail at the Printfield, Gilscomston, and the Hardgate villages, in the parish of the old town of Aberdeen, and not in the old town itself. But the mystery is explained, when I inform the reader that the mid-

wife, Mrs. Jeffries, who had all the practice of that town, was so very fortunate as not to fall in with the infection, otherwise the women whom she delivered would have shared the fate of others."

It appears, from this and other parts of his treatise, that Gordon fully appreciated the importance of being able to account for apparent exceptions to the principle he was working out, and that he proceeded with the caution characteristic of his countrymen in venturing to publish the very important conclusions at which he had arrived. It is impossible for any one to analyse Dr. Gordon's treatise fairly, for it is made up of the closest reasoning and the most distinct data. He never supposes anything, but looks carefully to see that his premises are correct, and leaves the reader to draw his own conclusions.

A few extracts from the work will show you what sort of a man he was. Of the nature of the poison, he says simply:—"With respect to the physical qualities of the infection, I have not been able to make any discovery." Of himself, he remarks: "It is a disagreeable declaration for me to mention that I myself was the means of carrying the infection to a great number of women."

He says of typhus, as compared with puerperal fever: "The cause of both is undoubtedly infection; but the two infections are of a very different nature." Of the cure, he says: "There is no disease in which less is done by nature or more may be done by art." As he based his treatment on the view, that the chief effects of the poison were of an inflammatory nature, he directs attention strongly to the benefits which follow early bleeding and purgatives. In his own words: "On this head, I speak with proper confidence, because I speak from experience—the surest test of medical truth. And, as I have already mentioned, I found myself disappointed when I trusted to those means which have been recommended by some authors of considerable respectability; for neither antiseptic nor tonic medicines, nor such as obviate sensibility or irritability, were found effectual."

I must not omit to mention that, as far as the pathology of his time allowed him, Gordon was correct in saying on that subject: "The puerperal fever is a disease which principally affects the peritoneum and its productions, and the ovaria". He distinguishes between "the most effectual means of preventing the infection from being communicated" and the method of "preventing the action of that infection after it has been communicated". He gives sensible hygienic rules to gain the first point, recommending in particular the purification of infected chambers and the fumigation of infected apparel. We can judge, now

that three-quarters of a century or more have passed, of the value of this work of Gordon's. It is just the result of that kind of rare combination of originality, energy, patience, and good feeling, which makes us dissatisfied with inferior productions, and is peculiarly characteristic of the leading minds of this country.

We might reflect a little upon the singular fact, however, that every point, except the most important one in this treatise, has been taken up, first by one author and then by another, and all have praised Gordon's work in the very respects for which he least deserved it. We might pause, I say, to reflect upon the question why it is so difficult to get rid of old notions and accept new ones, when they are true and important, or why it happens that error is so much more favourably received than truth when presenting itself as a candidate for support. With what kind of a feeling do we read the conclusion to Gordon's preface? "The benevolent reader must observe with displeasure the ungenerous treatment which I met with from that very sex whose sufferings I was at so much pains to relieve; for, while I was using my best endeavours to mitigate the calamities of many miserable sufferers, several others were busy in traducing my character, who, prompted by prejudice, very uncandidly proclaimed the deaths and concealed the cures, to raise an odium against my practice."

The interval between Gordon and Semmelweiss of half a century was occupied chiefly by those important investigations into morbid anatomy and pathology, both general and microscopical, which have contributed to the improvement of every department of our profession, and particularly to the practical knowledge of diagnosis and etiology. It is to this source that we must look for modifications in general opinion concerning some of the phenomena of puerperal fever; and it will be, I think, most convenient to defer our inquiry into the value of Semmelweiss's observations till we have reviewed the progress of pathological research. The real reason why there was such an universal movement in that direction about the end of last century may be easily traced to a few powerful minds, like the two Hunters and those nearly associated with them, who clearly perceived that the pathological study of the effects of disease promised more than the study of symptoms.

The result of this method, as far as it influenced the subject of puerperal fever, was the introduction of several new terms into our nosology which were intended to describe the pathological conditions of different organs, structures, and tissues. Thus originated puerperal peritonitis, phlebitis, metritis, and some others. These terms, as we know, did

not include any explanation of the remote cause which induced the changes observed by the pathologist. They all more or less involved the assumption, that what is termed inflammation was their exciting cause, and the practical object of these terms was to connect local inflammatory processes with local and constitutional symptoms.

The subject of the pathology of puerperal fever is an extensive one, and I shall not attempt to treat it in the same manner as that of the etiology of the disease. There is one point which would not be difficult to prove, if it were not self-apparent, and that is the fact, that neither morbid anatomy nor pathology assisted the theory of Gordon : I mean to say, that the idea of a specific poison would never have been evolved from such pathological work.

It was of very great value, however, in another way. Such theories as that which accounted for the swelling of the leg, or phlegmasia dolens, by supposed metastasis of lactical secretion, very soon retired before the scalpel of the pathologist. The result of all this is, that we have some chance now of succeeding, after the separation of many pieces which would not fit together, in completing a puzzle of really no great complexity.

If it were asked why Gordon's theory did not find acceptance with pathological investigators (for we may observe that there were some of these who were distinguished for an extensive knowledge of the disease in all its forms, and had studied carefully all its symptoms), we must look for an answer in the supposition, that the pathological mind is of such a character as never to feel satisfied of the existence of anything without direct evidence, and then only when the method of demonstration employed is one with which it is familiar. For example, suppose a person who is requested to look far into space, under the directions of another who describes to him an object which he is anxious to make him see. Perhaps a telescope may be employed, and yet the assistance which the one derives from it is no help to the other.

To what extent he who cannot see is likely to believe without seeing depends partly on his mental character and partly upon the confidence which he places in his friend's description. It is quite easy to conceive that, under such circumstances, it might be difficult to bring conviction to some minds, however well disposed. Now, the pathological mind is not a credulous one. It has a good deal of confidence in its own powers, and it likes to use its own visual organs. It would meet such a theory as Gordon's with an avowal of the greatest willingness

to believe in the existence of a specific poison, provided only it could be shown the poison. To imagine that it could be contented with less is to do injustice to its common sense and acuteness. For such a mind to conceive of an atom is almost impossible ; it appears to it practically useless to do so, and to range beyond the sharply defined cordon of the demonstrable seems to it scientifically reprehensible.

Let us examine to what pathology led. The principal effect was a very excellent classification of morbid changes, which has been followed closely by continental authors, and is still preserved by systematic writers in all countries. When it was found that descriptions of particular forms of inflammation were true for the particular cases in which they occurred, but not for all forms of the fever, we can understand that general views would soon embrace the apparently unconnected series of various morbid appearances which had been recorded. It soon appeared quite possible for various structures in the abdomen to be severally or simultaneously affected, or even that there might be well marked, though very different, pathological alterations, and yet that the cases were correctly described as forms of puerperal fever. Thus a simple classification was adopted under the general term uterine or puerperal inflammation, which included : 1. Inflammation of the peritoneal covering of the uterus and the peritoneal sac ; 2. Inflammation of the uterine appendages ; viz., the ovaria, Fallopian tubes, and broad ligaments ; 3. Inflammation and softening of the proper or muscular tissue of the uterus ; 4. Inflammation and suppuration of the absorbents and veins of the uterus. Each separate structure was thus carefully examined, and its alterations recorded.

There is a great deal to admire in the very exact and truthful descriptions, which make the works of the pathologists, to whom we are indebted for these valuable aids to our knowledge of puerperal fever, so well deserving of our regard. Probably the very fact, that a rigid exclusion of etiological influences in connection with these pathological conditions was observed by this school of pathologists, may be the cause of their work being of such great value. That publication of Dr. Baillie's, which contained nothing but careful and elaborate engravings, with the simplest descriptions possible, was the type of scholarly composition and masterly anatomical and pathological skill. There can be no doubt that, since the productions of that period from which we have traced the commencement of our history, nothing has ever appeared to compare with those of our own country. There were no uncertainty as to facts, no hasty generalisation, and no attempts

at artificial classification, amongst those that followed close upon Baillie's model. So carefully did they attach themselves to the firm rock of certain truth, and so sure were they of the ground on which they stood, that they would have wondered at many since their day who have been looking so long into the hazy atmosphere that surrounds those obscure regions of the origin of life and disease, as to have forgotten to move on whilst dreamingly attempting to define the misty outlines of forms beyond their power of perception or comprehension. Such is the feeling they would have experienced, combined with one of pity, if they had heard some of us say that the very foundations of pathology are uncertain; that we know not quite where we are, or what next we may see. This brings me back to an urgent claim for the careful preservation amongst us of our peculiarly national method of observation, which, while accurate in detail and cautious in expression, still considers the best proof of its value to be the benefit it confers by its practical application.

To return to our subject, we have to notice the important extension of the theory of inflammation to the explanation of the connection between that condition which was known as phlegmasia dolens, and certain morbid changes in the structures of the venous system of the uterus; that is to say, when the symptoms which had previously been referred to metastatic lacteal secretion or pressure upon the lymphatics, were found to depend upon obstruction to the venous circulation.

The term phlebitis assumes the possibility of the veins, as separate structures, being affected by inflammation. Various changes of their lining membrane, and the existence of pus within the vessels, were considered satisfactory evidence of inflammatory action; and then followed the theory of pyæmia or puerperal purulent infection, arising from suppurative phlebitis. We will not stop to discuss just now whether these views were altogether correct. There is no doubt that, as far as the facts were concerned, they led to very important results in the establishment of the theory of infection.

It was shown that phlebitis might occur, not only in the puerperal condition, but also as a consequence of suppressed menstruation; in cases, too, of malignant ulceration of the os and cervix of the uterus; in certain forms of tuberculous disease; also as a consequence of abortion, and after operations for fibrous and other tumours of the uterus. The theory of puerperal purulent infection was supported by the popular school of medicine in France, and was ably argued by the late Professor Trousseau. It never was very generally received amongst

ourselves, for the reason that we did not find it in accordance with clinical experience.

Trousseau entertained the idea that puerperal infection was always to be traced to a wound ; or that suppurative phlebitis might follow a wound, and pus might thus be introduced into the system. He assumed, or rather, fell back upon, an old theory, which has often been employed to explain many circumstances connected with the puerperal condition. As far as it bears upon our subject, which, I may remind you, is the specifically poisonous origin of the fever, we can fairly allow it to be passed over, at least at present. The theory to which I allude is this : that a certain diathesis characterises the puerperal state.

We can easily understand how the theory of a particular diathesis was also employed to explain that condition of the veins which we term thrombosis, or rather, that state of the blood which was supposed to be the primary cause of its coagulation ; and a further application of somewhat the same theory to tuberculous and cancerous cases, which assumed in the latter instance the more familiar title of cachexia, was conveniently used to explain the occurrence of the various pathological conditions, and the symptoms arising from them which were found to be common to these diseases as well as to the puerperal state.

The last edition of Trousseau's lectures was published some years after Semmelweis's work, in which, as we shall see, it was clearly shown that cases of carcinoma of the uterus in the same ward with puerperal cases might produce that fever. This was undoubtedly, though a simple observation, one of considerable importance in bringing clinical evidence into consistent accordance with pathological facts. There is one remark of Trousseau's, however, which clearly shews that he was quite aware of the weakness of his argument in favour of diathetic influence. "The same conditions", he says, "exist in nearly every recently delivered woman, or in all cachectic persons", yet "phlegmasia dolens is not a necessary complication in either class of patients"; from which he concludes very candidly that "consequently there exists a special cause which is unknown". It happened about this time that the doctrines of the German pathologists were exciting a good deal of attention in their bearing on the theory of inflammation. Those important changes which are observed in the various forms of puerperal inflammation were subjected to the theory which Virchow expounded, of the origin of inflammation in certain alterations in connective tissue. It is not essential to our argument to consider whether that theory be correct or not ; for it is, as far as we can judge at

present, not of very great practical importance whether the process of inflammation commences with active changes in the cellular elements of connective tissue, or in the exudation of the corpuscular elements of circulating fluids. They both agree in allowing the existence of some primary cause for the tissue-changes; they only differ in respect to the earliest effects which that cause produces. It is now generally admitted that the connective tissue is that which is chiefly affected in puerperal inflammation in all its various forms; that is to say, in metritis, phlebitis, and others; and the term cellulitis is more commonly used than any other, at least by those who receive the recent doctrines on inflammation.

Clinical experience and pathological examination of this question lead me to concur entirely in these views. Instead of regarding a vein as a separate structure, and thus admitting the term phlebitis to be pathologically correct, it appears that we must refer the coagulation of the blood, which is the consequence of so-called phlebitis, to changes in the connective tissue which surrounds the vessels. On this point, therefore, we are obliged to differ from the pathologists whom Trousseau followed, as well as from those who consider that the tissue-changes are induced by coagulation of the blood or thrombosis, whatever that may depend upon.

There is not much difficulty in comprehending the process by which a vein becomes filled with coagulum in phlegmasia dolens, whether it occur in the puerperal state, or under any other circumstances. Perhaps the best instance from which we can inform ourselves of the relation which the process of coagulation bears to that of cellulitis, is one of those rapidly fatal cases of the disease in which the large vessels which unite to form the vena cava ascendens are filled with coagula. By removing the main trunks contained in the pelvis, together with the adjacent arteries, as high as the diaphragm, we can determine by dissection the exact points where the coagula terminate. If we then examine the condition of the cellular tissue surrounding the vessel under inspection, it will be found that the external changes are in advance of the coagulum, assuming that both processes are taking place in the direction of the venous circulation. As a matter of demonstration, we may insert a needle through the vein at the point where the inflammatory changes are seen to terminate; and, on comparing its position with that of the termination of the coagulum, we may measure the distance between them. This may amount to an inch or more; but, to a great extent, it appears to depend on the number of the lateral veins that enter the vessel between those two points. The orifices of the lateral

veins are generally distinguished by a small projecting coagulum, which is much less firm than that of the principal coagulum. The space between the two points is not empty, but the blood which it contains is in the same condition as that which fills the vena cava itself, and generally distends the right cardiac chambers. It is satisfactory to find that this view of the pathology of phlebitis, using the term under correction, is admitted by the leading pathologists in Germany, and that they are inclined to adopt it in preference to the theory of thrombosis or primary coagulation.

We may dismiss this part of our subject with one or two brief remarks. It may be asked, How are we to explain the formation of pus in veins on the supposition that the connective tissue externally to them is primarily affected? We should reply to this: that there is evidence to prove that the different tissues of which the venous canals are composed participate very soon in the changes of the connective tissue; that the termination of the inflammatory process in any locality is the formation of pus, and that an abscess may form in such a position and in such a manner as to allow of a communication between the collection of pus and the venous canal. If, again, the question be put to us, Can the lining membrane of a vein secrete pus in the same way that the pleural membranes, the pericardium, or peritoneum are known to do? we can only answer that the epithelial surface of a vein may exhibit the ordinary changes which succeed one another in the stages of inflammation, and that in the final stage pus-corpuscles may be secreted, or rather developed, but only when the epithelial surface has been destroyed. The practical value of these pathological researches has been to afford us very valuable assistance, not in the elucidation of the etiology of puerperal fever, but in the explanation of several symptoms which were previously unaccountable, such as those resulting from embolism, coagulation of blood in the right side of the heart, the formation of secondary abscesses in various organs and tissues, and the constitutional disturbance which is produced by them.

I shall now request your attention to those contributions which were made in Germany to the etiology of puerperal fever by Semmelweis. Quite independently of his relative personal merits as a scientific observer, we may simply confine ourselves at first to the evidence which he offers on the subject of the contagious nature of the fever. After perusing the first few pages of his work, we find that he presents himself to us as an independent witness. There is also the manner which favourably impresses us with the idea, that he would not assert anything

which he could not prove ; on the contrary, we feel that he rather tires us with details. He does not mix up conjecture or theory with fact, though he may indulge too much in arguments which are intended to prove certain conclusions almost self-evident. The method of investigation pursued by Semmelweiss was much the same as that followed by Gordon, and the proposition he supported was, like his, that puerperal fever is a disease essentially produced by the introduction of a septic poison into the system. Gordon, as we have seen, declined to express any opinion on what the poison might be ; but Semmelweiss did not hesitate to state that it was generated in the foul organised substance which results from the decomposition of animal matter. The work does not contain any original contributions to pathology ; and in that respect, as an observer of pathological conditions, Semmelweiss appears to me to have been decidedly inferior to Gordon.

He argues his point somewhat in the following manner. "We see that the great lying-in institutions are singularly subject to occasional outbreaks of this disease, and the danger to puerperal women is very much greater in those institutions than in separate domiciles. In those hospitals where students are instructed, and are in the habit of coming to the wards, and to the examination of cases from the dissecting-room, the disease is more frequent and the mortality greater than when only midwives are admitted for instruction. To prove this, we have such and such a tabulated register of cases, which shows a mortality under one system of 10 per cent., as compared with $3\frac{1}{2}$ in the other. In Vienna, before 1841, the mortality in the lying-in hospital was small, when compared with that of a few years later, when the hospital had been converted from its first intention to serve for the clinical instruction of students."

"To prove that the poison was generated in the process of decomposition, and might be conveyed by the hands of attendants, a case of cancer of the uterus in a pregnant woman was the cause of a dozen fatal cases in the same ward, and the probable means of conveyance of the poison were the sponges and other things employed by the nurses, or in the course of examination by the medical attendants. We had a case, too, of necrosis of a knee-joint, which was also the cause of some fatal cases. These were clearly established facts, and they point to the same conclusion." This is the nature of the argument, which is fairly well worked out. To show that the dissecting-room might be the source of danger, he makes the declaration :—"I myself was appointed for the second time to the clinical assistantship of the first *clinique*, and

was in the habit of going into the wards after making explorations in the dead-room. The mortality in April and May was 18 per cent. and 12 per cent. respectively. During the four preceding months, I was only occasional assistant, and was not engaged in *post mortem* inquiries, nor was I obliged to examine the patients *seriatim* before the visits of the Professor. Look at the difference between the mortality during the two periods.

Then he goes on to show us from his tables that there is no connection between seasons of the year and the occurrence of the fever; so that it could not be said in any way to depend on epidemic conditions of atmosphere or temperature. There is a great deal more in the way of collateral evidence adduced by Semmelweiss, but the main line of argument he pursued is thus broadly indicated. It was essentially based on statistics fairly arranged, without taking advantage of such data, and without overstraining them.

He did not neglect an important test of his views by ascertaining the effect on living animals of the artificial introduction of healthy pus into the circulation, thus proving that the symptoms could not be referred to such a cause as simple purulent infection.

I have said that Semmelweiss's work consists chiefly of a collection of observations made by himself, without direct reference to the opinions of others. He does not omit, however, to mention the views of some few whom he was led to regard as the representatives of scientific thought, both in his own country and abroad. In this part of his work, however, it must be acknowledged that he gives proofs of possessing very limited knowledge indeed of the literature of his subject, and we cannot feel surprised that he finds but little difficulty in disposing of his opponents to his own satisfaction.

He brings his arguments together into a condensed conclusion at the end of his treatise in brief and forcible language. "I have shown", he says, "above that the excess of mortality in the first lying-in *clinique* in comparison with that in the second was occasioned by putrid matter, which was introduced by the hand of the examiner. I have shown that, in October 1847, the foul material of a foul cancer of the uterus produced puerperal fever. I have shown that, in November 1847, a decomposing material from a carious knee-joint produced puerperal fever; how the fever was connected with the visits of the surgeons in one hospital in particular, and how the mortality during six years was reduced to less than one per cent. when all connection with the surgical wards was broken off. It was the finger of the surgeon that

carried the poison in the latter case ; it was the atmosphere in the case of the knee-joint, and the air and the foul linen together in the *clinique* at Pesth, where the infection spread in spite of the washing with chlorine-water."

It may be mentioned that he had adopted the use of chlorine-water with signally good results, and derived from this fact increased support to his views of the septic origin of the disease. We will not venture to make any further comparison between the value of Semmelweiss's treatise and that of Gordon. As far as the merit of priority is concerned, though the observations and opinions of the one may have been quite independent of the other, we can certainly claim for Gordon precedence in date by a considerable period.

In their style of composition, in their arrangement of data, and their logical powers, it is more than probable that we should be inclined to claim for Gordon decided superiority over Semmelweiss. At the commencement of these lectures, I briefly alluded to the condition of mind which it is necessary for us to obtain in order to weigh fairly scientific evidence. It is easy to decide on the value of such evidence when we have tested it on the anvil of time. Observations and theories which have been conclusively analysed by that process naturally lose their interest. But, when we are studying the pages of history, it is the lives and characters of those who thought, and worked, and benefited us, that we feel an interest in contemplating, as well also as the lives and characters of those who did not think and did not work, and stood in the way to oppose the progress of scientific and practical knowledge. The history of this fever, if it contained nothing but facts, with the dates of their discovery, would not be a long one. The history of the erroneous ideas and statements which were advanced on the subject of its origin and nature would far surpass it in dimensions. It has been said by one who has displayed singular scientific acumen in philological research that, "for the discovery of truth, there is nothing so useful as the study of errors". It may be useful certainly to the individual who pretends to originality; but, for our purpose, I venture to think we have done well in confining our attention to the more limited object with which we started, of tracing the progress of truth through its various stages of development.



LECTURE III.

THE only point on which I now feel some hesitation is, whether this explanation of the origin of puerperal fever, and its contagious nature, is so generally admitted as to allow us to proceed in our inquiry without interruption by objections to it. Our students are generally told in the text-books that puerperal fever is "a continued fever communicable by contagion, occurring in connection with childbirth, and often associated with extensive local lesions, especially of the uterine system". (Aitken.)

There is no doubt that a considerable number of experienced practitioners believe in epidemic influences, and are quite ready to allow that there is strong evidence to make them conceive that in certain cases the fever has been communicated from one person to another. It is only a short time since that one of our leading journals, in remarking on a case of manslaughter which has excited attention very properly on the question of contagion, observed: "Is it certain that puerperal fever is contagious?" It seems to me a very important matter indeed that there should be no mistake on this question, at least as a practical one; for, whether we believe or disbelieve the theory of contagion, we must regulate our practice by the opinions of those of the greatest experience and knowledge.

Supposing even that we cannot demonstrate the perfect truth of the theory, and that we cannot explain certain apparent exceptions to it, it is necessary to remember that it is sufficient, if the proof can be carried so far as to obtain consent to the probability of the theory being true. In this case, "probability is the very guide of life"; and the rules laid down by an able writer, on the extent to which we ought to be satisfied with the evidence of such probability, are so much to the purpose, that I shall take advantage of them to express the opinion, which I think we shall agree in supporting, in respect of this matter.

"From these things, it follows", says that able logician Bishop Butler, "in questions of difficulty, or such as are thought so, where more satisfactory evidence cannot be had, or is not seen, if the result of examination be, that there appears, upon the whole, the lowest presumption on one side, though in the lowest degree greater, this determines the question even in matters of speculation, and, in matters of

practice, will lay us under an absolute and formal obligation, in point of prudence and of interest, to act upon that presumption or low probability, though it be so low as to leave the mind in very great doubt which is the truth. For surely a man is as really bound in prudence to do what, upon the whole, appears according to the best of his judgment to be for his happiness as what he certainly knows to be so." (Butler, Introduction to *Anal. Relig.*) It is quite easy to perceive what a great difference must exist between the views entertained by one who believes thoroughly in the contagious theory and those of one who does not believe in or is doubtful of it. By the former, epidemics of the fever are not admitted; symptoms are all referred to the influence of a poison circulating in the system, which accumulates in tissues and produces general disturbance as well as local pathological changes. Variations in the pulse and temperature are explained by the same cause, and the remedies which appear most rational depend upon the principle of opposing the poisonous influence, while the fear of conveying septic matter from one case to another will always suggest precautions which would be disregarded by one who doubted such a possibility. In regard to the question of lying-in institutions, and in his views of the dangerous proximity of the *post mortem* room, a believer in the contagious theory would hold only one opinion. In many other ways which need not be mentioned, his professional conduct would be regulated in regard to various cases which might occur in his practice of those diseases which are also the source of the infecting poison. If he engages in discussion on these practical questions, he carefully declines to argue on the meaning of the term contagion. If it be stated that puerperal fever, in a large number of instances, cannot be traced to any source of infection, he replies that his theory does not involve such a necessity; that the symptoms may arise from the decomposition of organic matter in the patient's own person; and he does not allow the limited meaning of the term contagion as used in such diseases as scarlatina, variola, and others, to be adopted in the case of puerperal fever; and, so far as such diseases occurring in the puerperal state are more serious than under other circumstances, he would either assert that he doubted the fact, or that, if it be occasionally true, he would still adhere to the opinion, that such increase in the severity of symptoms might be explained upon the principle of septic infection.

There may undoubtedly be some demand upon the imagination for those who find it difficult to conceive of invisible and subtle agents of

whose existence there is only the evidence of effects ; but, when the mind is educated by acquaintance with many natural phenomena that can only depend on unseen forces or energies, that difficulty diminishes, and a consideration of the extraordinary character of the effects produced by the action of all poisons on the human system will assist in allowing the same method of reasoning to be employed which is pursued in the investigation of the laws which regulate the action of chemical, physical, and other forces. That those who are engaged in other departments of scientific investigation are required to exercise the imagination, when they have arrived at the limits of actual perception, is proved sufficiently by the following remarks of one of the leaders of experimental research in the domain of natural philosophy.

“ I would simply draw attention”, says Professor Tyndall, “ to the fact, that in the atmosphere we have particles which defy both the microscope and the balance, which do not darken the air, and which exist nevertheless in multitudes sufficient to reduce to insignificance the Israelitish hyperbole regarding the sands upon the sea-shore.” “ It cannot be too distinctly borne in mind that, between the microscope limit and the true molecular limits, there is room for infinite permutations and combinations.” (*Microscopical Journal*, vol. x, p. 416, new series.)

It is quite clear that, if the efforts be successful which are now being made to demonstrate the actual existence of organic poisons developed in the process of decomposition, we shall some day be enabled to account satisfactorily for the different symptoms which they give rise to when introduced into the human system, and for the pathological changes they produce in the different tissues of the body.

This question of the different characters of the products of decomposition is, of course, extremely difficult to decide. We have only to be careful to require that observations should be accurate, and that theories should not be hastily proposed. It might appear quite an easy matter to investigate the results of the decomposition of any particular organic substance, and determine the sensible properties of the minute organisms which are developed in that process ; I say it might seem easy to do this ; but practical observation soon dispels this idea. The field of the microscope is seen to be crowded with particles of extreme minuteness, of variety of form, reflecting and refracting light in various degrees, and exhibiting movements of more or less complexity. Those movements may be independent of the organisms themselves ; that is to say, they may be produced by external forces. Thus, if we employ

only our powers of vision, though we may magnify to any extent, as a means to discover their characters, and to distinguish them from one another, we are only using morphological principles as the basis of classification.

This limitation of their powers of analysis with the microscope none are so well aware of as the most experienced observers, and there is but little danger of our being led to erroneous conclusions by them. Those who are least experienced in the difficulty of microscopic research are more likely to exceed the limits of observation, and to indulge in theories which will prove eventually to be only partly true.

It would be difficult to do justice in these lectures to the researches which have lately been made into the interesting and important subject of decomposition. A learned Fellow of this College has fairly stated the general results of the most recent investigations of Continental observers, after verifying them by independent examination, as well as contributing some information of value himself in his recent lectures at Owens College, Manchester (*vide* BRITISH MEDICAL JOURNAL, January 26th, on the "Occurrence of Organic Forms in connection with Contagious and Infectious Diseases"); and his conclusions are more in accordance with all that we know from experience, clinical observation, and experiments, than could be expected from one who, like the distinguished Professor at Jena, has made the study of the different species of organisms, and their development, almost his sole object of inquiry.

It is only necessary to select such of these observations as bear directly on the symptoms and pathology of this subject. Those who are unacquainted with the general characters of the minute organisms which are classed under the term bacteria may readily obtain some idea of their dimensions, forms, and other distinguishing features, by the examination of any fluid containing decomposing organic matter. For instance, the secretion in carcinoma uteri may be subjected to microscopical examination with the $\frac{1}{4}$ in. or $\frac{1}{8}$ in. objective; and, instead of a simple collection of bacteria, we have in this case the cells of tissues with which we are tolerably familiar, to assist us in forming some idea of the extreme minuteness of those organisms. A cancer-cell appears to be capable of containing quite a considerable number of the darkish globular granules, which are now looked upon as special forms possessing active infective properties. We can readily recognise in the fluid obtained from a decomposing cancer of the uterus the staff-shaped bacteria, properly so-called, with their undulating mode of

progression, the spirilla with rapid axial rotation, the more obscure movements of the vibrios, and, lastly, the comparatively insignificant globular micrococci. It is the last of these which, under special names, as *micrococcus diphtheriticus*, *micrococcus vacciniæ*, and others, which are not so easy to distinguish from one another as to make us quite satisfied of their specific character, that are now engaging the attention of pathologists in relation to the various changes of tissue which are met with in puerperal disease.

There is no difficulty whatever in conceiving it quite possible for even the larger bacteria to traverse the smallest capillaries; for we have only to introduce under the cover of the microscopic slide a few red blood-corpuscles, in order to perceive that three or four staff-shaped bacteria placed side by side could follow a blood-cell in its passage through minute vessels. And, when we observe that a micrococcus is a sphere whose diameter is not equal to that of the staff-shaped bodies, we can imagine it capable of exerting local morbid influence by interruption of the physiological processes, which are constantly taking place in the body.

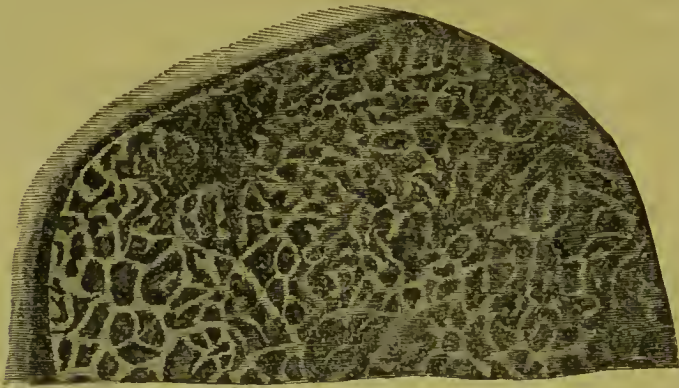


Fig. 1.—Section of Pleural Membrane, with network of Lymph-vessels full of Bacteria. Natural size (Heiberg).

My object in presenting in this familiar manner the general characters of bacteria is rather to favour the view that, instead of treating them with contempt, we may not only appreciate the fact of such organisms existing, but may give some scope to the imagination when we are considering the question of morbid poisons, and are asked to believe in much that cannot be brought within the range of actual perception. It might be reasonably objected that there is great liability to error in microscopic analysis of the characters of micrococci, and that we might easily mistake other cellular forms for them. This is an objection

which is perfectly true, and against which the microscopist has to guard very carefully.

At present, there seems to be one test which may be applied in pathological examination of tissues supposed to contain micrococci, and this is required by the fact, that the minute globules of oil commonly met with in all tissues in certain pathological conditions resemble micrococci so closely as to make it difficult to distinguish between them. This has suggested the immersion of such tissues in strong solution of caustic potash, which is known to have no effect upon the microscopical appearance of a micrococcus.

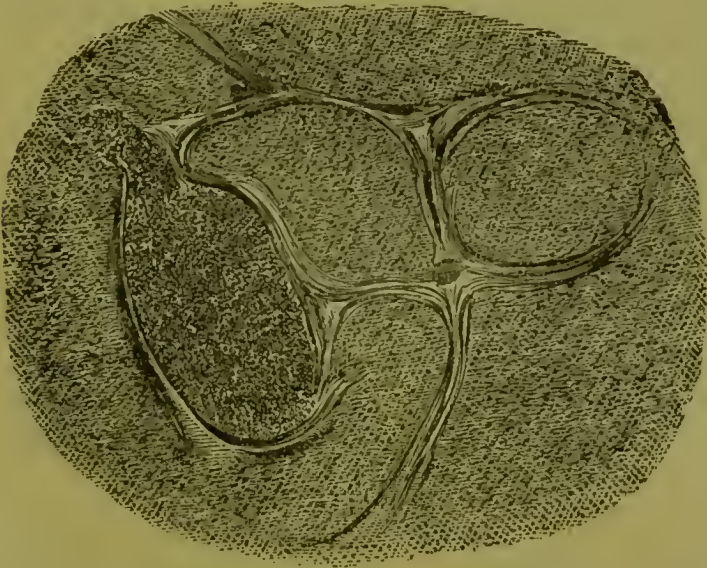


Fig. 2.—Section through a Bacterian Embolon in a minute Artery in the Kidney.

The patient sank very rapidly after delivery, and the *post mortem* examination led to an explanation of the symptoms in the gangrenous condition of the placental surface, and venous thrombosis in the tissues which correspond to that part. In the lungs and heart, abscesses containing bacteria were found (Heiberg).

By these means, Professor Heiberg of Christiania (*Die Puerperalen und Pyämischen Processen*, Leipzig, 1873) has lately succeeded in satisfying those who have had an opportunity of examining his preparations, and who are qualified to form an opinion upon their value, that, in cases of puerperal fever, very many tissues may contain bacteria. He has exhibited specimens in which the lymph-canals of the serous membrane of the liver are seen to contain them in small groups or "colonies"; and they are not confined to the canals, but seem to have permeated their walls and invaded the external tissue. Similar groups are found in the lymph-vessels of the pleural membrane (fig. 1, p. 38);

in the tissue of the cardiac valves, which is rendered slightly opaque and swollen around them; in the arterics and tubes of the kidney (figs. 2 and 3); and, among other results of investigations, Professor Heiberg describes them in those lacunæ which exist in the nasal mucous membrane, and communicate by fissures with the surface.

We can easily apply these facts to the explanation of the formation of minute abscesses around arterial embola, and to the different pathological conditions which follow a simple fibrinous coagulum arrested in the circulation, as compared with one containing active micrococci. This is the direction in which pathological investigations will be made by those who support these views of the connection between bacteria and puerperal fever.

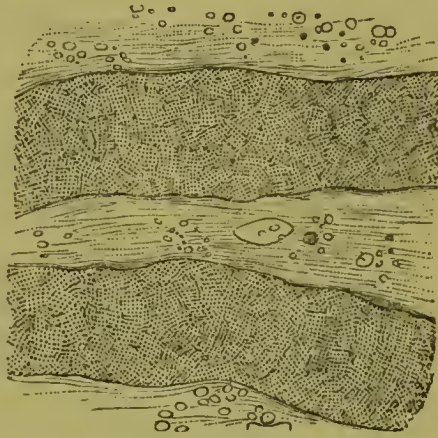


Fig. 3.—Canals in the Pyramidal Portion of the Kidney full of Bacteria. Between the canals, the stroma of the kidney contains some fat-globules. The specimen was taken from a patient who died about four weeks after delivery from puerperal infection. Numerous abscesses with which lymphatics communicated were found in the periuterine tissues. Both kidneys were large, pale, and soft, and contained streaks and spots of yellowish colour, and numerous points of ecchymosis. The liver was slightly enlarged, of paler colour than normal, and the divisions of the acini were indistinctly defined. The pericardium was spotted with points of ecchymosis, and its serous cavity contained sanious fluid (Heiberg).

The idea of searching for micrococci as Professor Heiberg did was suggested by reflection on the difference between one embolus and another, that is, from a comparison between the state of the tissue surrounding small collections of healthy pus, which had been injected into the veins, and had been arrested in the minute branches of the pulmonary artery, and the much more active changes which resulted from the injection of material containing bacteria.

It was in a case of ulcerative endocarditis occurring in pyæmia that Professor Winge first referred to the existence of micrococci in the pathological changes observed in the endocardial membrane, Professor

Virchow having previously described this particular form of ulceration in puerperal fever as of diphtheritic nature. This case of Winge's was followed by another, the details of which were supplied by Professor Heiberg, and was accompanied by a postscript by Virchow, in which he refers the species of micrococcus to the genus *Leptothrix* of Hallier, expressing the necessity of caution in deciding upon this difficult point, as the very minute size of the corpuscula, and the absence of movement in the particles, necessarily make the microscopical examination somewhat uncertain. However, he states very positively that they are parasites, and are related to diphtheria and blood-infection. This is the present state of our knowledge regarding bacteria in their relation to puerperal infection ; and, as there seems to be good reason for believing in the facts as far as they go, we might decline to speculate upon the question which presents itself, Whether these micrococci are active agents themselves in producing the effects described, or whether they are only the vehicles of a subtle poison which attaches itself to them as well as to any other particles of matter, whether micrococci or not. Or suppose that we question the specific characters of the micrococcus, and ask whether there may not be others of equally active properties whose presence we cannot detect in the fluids or tissues. When we know how many varieties of bacteria are developed in the process of decomposition, we should feel inclined to doubt whether one and all might not be equally capable of producing poisonous effects when introduced into the system.

When Professor Lister remarks, "We now see it is not essential to assume the existence of a special virus at all, but that organisms common to all the sores in the ward may, for aught we know, assume specific properties in the discharges long putrefying under the dressings" (*Microscopical Journal*, vol. xiii), it is clear that he is indisposed to allow that a poison apart from bacteria can be brought into existence. It is not really because these points, so difficult at present to decide, are of practical importance that I refer to them. But they are interesting, as showing pretty clearly where the various parties of investigation have started from in search of fresh knowledge. I have claimed the privilege of testing all reports thus returned by the rules of general experience, supposing there is any great difference of opinion as to their correctness. There has been little occasion, however, to do this as yet ; for the most valuable contributions we have received are the results of the labours of men of practical experience in the clinical phenomena of the disease produced by septic infection. It seems to

me that, for this reason, we may accept the conclusions of Virchow, Lister, Billroth, Heiberg, and others, who are quite able for themselves, and are in the habit, of applying to their researches the test of clinical experience.

There is one case, for instance, in which we might find it difficult to explain, by the theory of bacteria, certain morbid phenomena, if we confined ourselves to the limited effect which the particles produce on the tissues: those changes, namely, which we term inflammatory. It is well known that there are cases in which we find no collections of pus, or secondary abscesses, and no well marked pathological alterations, cases usually of the acute form of puerperal fever, and where the constitutional symptoms have been the chief evidence of septic infection. In such instances, morbid anatomy reveals conditions not unlike those which we meet with in certain other contagious diseases: enlargement and softening of the spleen, congestion of the mucous and serous membranes, fluid condition of the blood, and slight general congestion of the pulmonary tissue. It is in these cases that Heiberg felt a difficulty in retaining the use of the term pyæmia; for there is neither a primary focus of purulent matter, nor are there any evidences of the presence of pus in any part of the body.

This is the reason why Professor Billroth uses the term *septhæmia* to signify the resorption of septic stuff, and *pyæmia* the resorption of pus. We have also *septicæmia* contrasted with *ichorrhæmia* in some recent German works. These terms are intended to separate cases of acute septic infection from those of longer duration; for, as it is well known, in cases of abortion, retained placenta, and others, where the origin of the decomposing substance is not very doubtful, we observe a series of symptoms which lead to the belief that a slow process of poisoning is going on in the system of the patient. Such a process would be termed *ichorrhæmic*.

If we follow Professor Heiberg, as certainly we are inclined to do, on the grounds of clinical experience, we should regard these puerperal processes, in whatever form they present themselves, and whatever characters the symptoms and pathological changes may assume, as depending on one and the same cause, the action of which on the nervous system, the organs of circulation and respiration, on the temperature, and on the fluids and tissues of the body, may be modified by circumstances similar to those which we know to be capable of modifying the action of poisons generally.

For some reasons, therefore, we might feel disposed to rest satisfied

with the old term putridity, which involves no such theory as septicæmia, pyæmia, and others; or we might fall back on the simple classical term sepsis as a convenient one for general use. In the clinical observation of cases in which septic agents are more or less actively at work in the system, whether in simple puerperal fever, in cases of abortion or retained placenta, in carcinoma uteri, or under any other circumstances, we are led to divide our attention between local and constitutional symptoms; that is, between local pathological changes and general constitutional disturbance.

This evidently becomes necessary for the purposes of treatment. Allowing that the local action of septic agents is the production of those changes of tissue which we term inflammation, the question arises whether there are other causes for the same effects as well as septic material.

This question is answered by some in the following manner. "It is possible that inflammation may be excited by various causes other than the presence of bacteria; and we would distinguish the former from the latter by the term simple traumatic pyæmia and specific traumatic pyæmia, pyæmia being used in the general signification of sepsis. From simple traumatic pyæmia may follow metritis, endometritis, parametritis, perimetritis simplex, or diffuse peritonitis. In surgical cases, this disease assumes the form of erysipelatous rubor around the wound; that is to say, conditions depending on local causes. Phlebitis and thrombosis may also be consequences of simple traumatic pyæmia. The only difference between these and the specific form of pyæmia is the presence of bacteria." These are the views of Billroth, Heiberg, and others.

Now, it does not unfrequently occur that cases present themselves in which we find only those alterations of tissue which are attributed to simple traumatic causes, and yet the history and other circumstances of these cases are the same as those in specific traumatic inflammation. What must always be a difficulty appears to me to be the diagnosis between the one and the other form of inflammation. It is difficult, I think, to obtain a very clear idea of what simple traumatic inflammation is, as distinguished from the other. Does it not seem that we are defining it by negative characters; in fact, that, instead of being simple, it is a very complex expression, for it may be anything; that is, any substance or force except bacteria? If we are to call one simple in its nature, the most definite ought to have the first claim to the title. We are aware that, pathologically considered, there is no

difference, as far as we can tell at present, between cellulitis or phlebitis of the thigh, as it occasionally occurs in men, or as we meet with it in the puerperal condition in women; so that, in any particular case, we are obliged to depend upon the history for information as to the origin of the disease, and upon the general symptoms for evidence of septic infection.

This is the clinical aspect which the subject presents, and, regarded on a practical point of view, it appears doubtful whether the division proposed is consistent or useful. We may have cases of metritis, which have originated in abortion; but, by active treatment, the decomposition of organic matter within the uterine cavity has been prevented from infecting the system, and no symptoms are present which enable a diagnosis to be made between simple traumatic metritis and specific metritis. It may be remarked, that the difference which is assumed and appears to exist between simple and specific inflammation was present to Cullen's mind when he observed: "Whether there be any belonging to the order of phlegmasia is doubtful." (*Op. cit.*, p. 80.) In speaking of erysipelas, he remarks: "It is doubtful if this disease be properly, in nosology, separated from phlegmasia." (*Op. cit.*, p. 377.) Or, again, "It is probable that an erysipelas is sometimes attended with, or is a symptom of, a putrid fever." (*Op. cit.*, p. 379.) Looking at the question from a common sense point of view, we may reasonably ask whether there is not a danger of making confusion in our views of the symptoms, pathology, and treatment of the disease we are considering by allowing the use of an indefinite or ill-defined word like "traumatic" into the terms of definitions. It sometimes happens that we can discern the flaw in an argument or proposition without much trouble; at other times, we are withheld from consent by a feeling that, though the logical process by which the conclusion is arrived at be correct, there is a certain want of character in it which makes us hesitate. If we take the trouble to examine carefully any proposition of this kind,—and we know how frequently they present themselves to the physiologist and physician,—we shall find that there is some slight incorrectness in the premises of the argument. It is quite open to discussion whether metritis in all its forms, which are said to be traumatic, may not have been occasioned by the influence of an organic poison developed under certain conditions, or of external forces, such as cold, injury, and others. It is not difficult to perceive the direction which such reflections must give and are giving to pathological research. To return, however, to puerperal fever—a disease

which, we are induced to believe, is to be explained in all respects by the influence of septic poison—it would appear that, in most cases, we are to search for the focus of decomposition in the cavity of the uterus or the surface of the mucous membranes which are connected with it. Whether we can prove that there is organic substance undergoing decomposition or not, we are to be satisfied by the symptoms which are present that such is the case. We are also required to admit that, under several conditions, all of which involve the presence of septic substance, symptoms similar to puerperal fever may be developed ; and that, whatever differences are observed in the acute or chronic character of such symptoms, we are to refer them to the rate of admission of the poison into the system. We assume that we may explain the peculiarly acute form of puerperal fever with which we are acquainted by the fact of the existence of venous sinuses in the walls of the gravid uterus, and of large lymphatic vessels by which the septic material easily enters the circulating fluids ; and, when asked to define what we mean by a septic agent, by sepsis, or by any other term involving the same idea, we may make the basis of our definition the existence of active living organisms resulting from the decomposition of organic matter, having special microscopical characters, producing special effects on the system of man and beast, and the transmission of which is regulated by ascertained principles.

It is time now to consider the application of our general conclusions to the subject of the treatment of the febrile puerperal processes, local and constitutional. The first point to which we naturally turn our attention is the determination of the conditions under which decomposition proceeds most actively, and what chemical or other agents may be employed with most certainty to arrest it. In his arrangement of what Cullen termed indication of cure, the details of treatment are divided into three classes. We have directions in the first “to moderate the violence of reaction”; in the second, “to remove the cause or obviate the effects of debility”; and, in the third, “to obviate or correct the tendency of the fluids to putrefaction”. The principles which were observed in the enumeration of remedies contained in classes one and two were consistent with the views which regulated Cullen’s practice in all febrile diseases, whether contagious or not. The directions contained in the third class are more in accordance with the views we now entertain of the origin of puerperal fever.

We are “to obviate or correct the tendency of the fluids to putrefaction” by—I. Avoiding the application of putrid or putrescent matter by

(a) removing the patient from places filled with corrupted air ; (b) correcting the air, from which he cannot be removed ; (c) avoiding the accumulation of the patient's own effluvia by a constant ventilation, and frequently changing the bed-clothes and body-linen ; (d) removing carefully and speedily all excremental matters ; (e) avoiding animal food, or correcting it. II. Evacuating the putrid or putrescent matter already present in the body by (a) evacuating frequently the intestines ; (b) supporting the excretions of perspiration and urine by (1) diluents ; (2) neutral salts, antiseptics ; (3) fixed air. III. Correcting the putrid or putrescent matter remaining in the body by diluents. IV. By resisting further putrefaction, or obviating its effects, by supporting the tone of the vessels by tonic remedies.

It does not really appear to be possible to add much to these details. We may, however, in one respect consider that we are in a much better condition to deal with puerperal fever than Cullen was, for the reason, that we have the means of putting into force his indications more thoroughly than he could. In consequence of the experiments which have been made to determine the conditions under which decomposition and the development of bacteria proceed, we are now supplied with agents which we may use to good purpose in the practical treatment of all cases in which we have reasons for thinking that there is a localised focus of infection.

These experiments are sufficiently interesting to deserve careful attention ; for it seems reasonable to attach much greater importance to the arrest of the local development of bacteria than to attempt to combat their influence upon the system. There are two methods of analysis which suggest themselves for the determination of the value of any particular substance as a disinfectant. We may take a solution of a fluid containing any organic matter known to decompose under certain conditions ; and, before that process has commenced, we may mix with the solution some of the substance or fluid whose properties we desire to know. By comparing the condition of the organic solution thus treated with some that has been allowed to develop bacteria freely, we may ascertain the disinfecting properties of the substance in question. The other method is the addition of solution of the disinfectant to solution containing active bacteria. By microscopical examination, we can readily ascertain from the diminished activity, or the sudden cessation of all movement in the bacteria, the influence of the substance we have employed. These are, generally speaking, the simple methods which have been adopted as the best means for affording an

answer to the practical question of the value of disinfectants. Now the same test is applied in both cases ; that is to say, we employ the microscope to examine what evidence there is of decomposition, or the existence of bacteria in fluids treated by the first method, as we do to ascertain the direct effects observed in the second method.

In his recent work on *Coccobacteria Septica*, Professor Billroth seems to have preferred the first plan. For instance, he takes solutions of the carbonates and sulphates of alkaline and mineral bases of definite strength, and adds them to the organic fluid, keeping side by side with the mixture, under exactly the same atmospheric conditions, and in these experiments, at the temperature of the human body, some of the unmixed organic fluid. Then, at intervals of a few hours, a few drops of each fluid are removed, and the bacteria forms which are present are registered. Thus time, to some extent, is admitted into the analysis, so long at least as there is any reason to expect the further development of bacteria in the fluid under examination.

Billroth states his general conclusions regarding certain substances as follows : " The abundant addition of carbonate of potash, sulphate of potash, sulphate of iron and copper, boracic acid, and borate of soda ; chromate of potash, aqua plumbi, carbolic acid, lactic acid, and butyric acid, hardly allowed the coccobacteria vegetation to take place. . . . Hypermanganate of potash (ten grains to a fluid ounce of water) had no influence." The term "hardly" is meant to imply that, if bacteria were developed, they were only the most minute and the lowest forms, and that, beyond this stage in the process of decomposition, there was no progress. The solutions of the alkalies used by Billroth were of the strength of a teaspoonful of the salt to an ounce of water—a combination which would not be convenient as a disinfectant applied locally.

The remark, that the hypermanganate of potash had no effect, has been often made before, and only proves that the chemical theory of disinfection must not be relied upon in the estimation of the value of disinfectants. There is so little difficulty in making a considerable number of examinations in a short time by the second method, that we may easily satisfy ourselves of the various effects of different substances, but in various degrees of dilution. For example, we all know that carbolic acid has active properties, both in preventing decomposition and in destroying organic life. But, when the clinical question has to be decided, What degree of dilution may we reach without depriving the carbolic acid solution of its properties, we are obliged to resort to

observation of the direct effects of solutions of different strengths on living bacteria. I have found that a convenient plan of making such observations is to place on the same slide a drop of bacteria fluid at a distance of about an inch from a drop, let us say, of carbolic acid solution; to cover them separately with films, and, while keeping the movements of the bacteria under inspection, to bring the two films into contact. The fluids beneath soon mix with one another, but sufficiently gradually to allow one to observe the effect on the animalcules. Thus, by taking solutions of carbolic acid of graduated strength of one in 40, one in 60, and so on, up to one in 120, we can determine which is the best to employ in clinical treatment. We thus ascertain that the last of these has a distinct but not powerful toxical effect upon living bacteria; but that, beyond this point of dilution, bacteria exhibit no decided disturbance. For practical purposes, we should be inclined to use the dilution of one in eighty, as we find that it is quite powerful enough to destroy activity, and is not liable to produce irritation when applied to sensitive tissues. Similarly, we ascertain that, for the liquor sodæ chlorinatæ of the *British Pharmacopœia*, a valuable disinfectant, the dilution of the fluid with two of water furnishes us with a reliable disinfectant.

It did not appear to me to be desirable to enter at any length into the various questions which are associated with the subject of disinfection beyond determining practically the most convenient material for the purpose. At one time, it did not occur to me to make any great difference between the solutions which we accept generally as useful disinfectants; but, when attention is directed in that serious and painful disease, carcinoma uteri, to the existence of bacteria as the probable cause of unexpectedly early death, it becomes apparent that we may prevent infection, if we cannot arrest the progress of cancerous disease, by attention to details. I might have added a few remarks on one or two other substances which have appeared to be advantageously employed in combination with carbolic acid, and especially the solutions of opium. Apart from the sedative effect of the drug, it would appear that it has a special disinfecting property, not very powerful, but decidedly exhibited in such a mixture as one part of liquor opii to three of water. There is no very definite time for the formation of bacteria, as can be easily understood, in cases of septic infection: I mean to say that, in some cases, decomposition seems to proceed more rapidly than in others. From general observation in those cases of carcinoma uteri which are characterised by the most severe symptoms, the effects of infec-

tion may be observed to take place in between six and twelve hours, and those in constant attendance on such cases consider that a space of four hours is quite sufficient to allow of the generation of multitudes of bacteria. The same may be said of cases of retained placenta or decomposing uterine fibroids.

The more we attend to the satisfactory results of the practical application of the theory of infection to the treatment of symptoms, the less important does the special influence of antiseptic constitutional remedies appear to be. There are some who conceive that opium is a valuable agent in the constitutional treatment of the puerperal fever. It is not to be denied that its sedative properties may be employed to great advantage for some symptoms ; but, regarded as a constitutional antiseptic, experience does not prove that it enjoys any special virtue. It may be observed that, if the introduction into the system of substances like carbolic acid be attempted, on the theory that the fluids of the body may be so far impregnated with the disinfectant as to prevent the action of bacteria, it is necessary to show that such a quantity as could at a minimum reasonably effect such a purpose must be capable of being absorbed, or can be administered without injury.

On the subject of the treatment of local inflammatory symptoms, it is unnecessary to make any particular remarks. There are one or two questions of importance which I have avoided, for the reason that they are not directly involved in the question of the specific nature of the disease. One of these is the connection which we have reasons for believing exists between puerperal fever and erysipelas, and other diseases which appear to have a similar origin with it, if they be not identically the same. Many instances are on record, and many more are preserved in the recollections of experienced practitioners, of the fact, that puerperal fever, phlebitis, and erysipelas are producible from one another presumably through the medium of the atmosphere. It has been my object to ascertain whether there were any *à priori* objections to such opinions rather than to dispute the facts, or oppose them by counter-evidence. But theoretical considerations fully support the results of experience, that erysipelas and puerperal fever, and all the forms of cellulitis, may be produced from one another ; and that their origin is the same. The practical study of these maladies, in their connection with one another, seems to offer the promise of explaining much that we previously failed to understand.

There is another point on which I speak with some hesitation, for the reason that I am not in a position to present conclusive evidence

upon it. It refers to the question of the treatment of that complication of puerperal inflammation known as thrombosis, as well as one of the accidents of thrombosis, namely, coagulation of blood in the pulmonary artery and the right cardiac cavities, which is accompanied by a rapid fall of temperature occurring simultaneously with the distressing symptoms of this accident. This symptom has been recorded by different observers, and, as the explanation of it would not seem to be a difficult problem for the physiologist, I shall confine my remarks to the treatment of those cases in which it may or has occurred, and to the best method of relieving the pulmonary and cardiac symptoms. The theory that thrombosis depends on spontaneous coagulation I have given reasons for believing to be pathologically incorrect, at least where we find such pathological conditions in the tissues external to the veins as to account, on mechanical principles, for the coagulation. It appears that the pulmonary and cardiac symptoms referred to are to be explained by the detachment of a portion of the coagulum in a distant vein, and its arrest in the pulmonary artery; that from this point the process of coagulation extends in the direction of the right ventricle, and gives rise gradually to the formation of the large coagulum which we find in the right cavities.

From clinical experience, we are led to conclude that, while the process of coagulation is limited to the veins of the lower extremities, and has not extended above Poupart's ligament, the possibility of a portion of coagulum being detached is very small, for the reason, as we have already seen, that by the course of inflammatory changes the current of blood from lateral vessels is prevented from impinging upon the coagulum. But, where the branches of the intrapelvic vessels join those from the extremities, there is a great probability of a portion of the coagulum being removed; and, on referring to such cases, it is found that the accident has happened when the patient has so far recovered from acute symptoms as to allow of a certain amount of movement. This suggests, of course, the necessity of great care during the period of convalescence.

The effects of thrombosis of the pulmonary artery are of such a serious nature as to make it desirable to ascertain what is the best mode of treating its occurrence. We have remarked that it is difficult to make observations intended to test the credibility of the opinions of others; and, when such a theory as that which leads to the administration of large doses of alkalies, with the object of arresting coagulation on chemical principles, was offered to us some time since, with some

interest, I watched, under personal direction, the effects of this treatment, as well as the practice of others—a method of substitution which is highly conducive to impartial conclusions. These were not in favour of the theoretical grounds or practical benefits of the mode of treatment in question. Speaking from personal experience, I have no hesitation in saying that it is better to regard the occurrence of coagulation in the pulmonary vessels, and its extension to the heart, on simple mechanical principles. In proof of the correctness of this view, I considered it was justifiable, in a case of this kind, where coagulation had taken place in both lower extremities as high as Poupart's ligament, and, on one side, had entered the pelvis, to use rather active measures to relieve the cardiac symptoms. They had occurred quite suddenly during the period of convalescence, and were characterised by the tumultuous action of the heart, distress in respiration, and diminution of temperature (in this instance, from 100 deg. Fahr. to 96.5 deg. Fahr.), which are diagnostic of pulmonary embolism.

The relief which followed rather copious bleeding by the application of leeches was very considerable, both when the symptoms occurred for the first time as well as on a second occasion, when they recurred in a less severe degree a few days later. The patient ultimately recovered. We cannot, of course, decide much from one case; but it may be generally remarked that, as a local remedy for the relief of the pain which accompanies active inflammatory processes in the cellular tissue of the limbs or of the abdomen and pelvis, there is none so certain as local bleeding. We are discussing principles, however, and not details of little interest for those who regulate their practice by the scientific theories of medicine.

I have reviewed in a very general and, I am conscious, in an imperfect manner this subject of puerperal fever; I have endeavoured rather to establish some solid basis for the practical treatment of the disease than to investigate those difficult questions which are now engaging the attention of scientific observers. It may be said that, after all, we have not made any great progress since the time of Cullen, nay, since the Hippocratic period, for the theory we have been arguing for is not essentially different from the “putridity” of the one or the *σήψις* of the other.

The idea of atoms is old enough, and many, there is no doubt, may feel inclined to distrust attempts like Heiberg's to reduce all the phenomena of puerperal fever to the existence of micrococci in the various organs of the body. Those who distinguish facts from ideas

will, it is probable, continue their labours, or give their encouragement to all investigations bearing upon such an important subject, feeling confident that our profession and society will some day be benefited by them.

As Professor Heiberg remarks, we are only just commencing to obtain some knowledge of a class of living organisms which threaten to disturb the principles of medical science, and appear to endow with practical existence the ideas of the natural philosophers of bygone ages. In tracing the history of the doctrine of the nature of puerperal fever, there was, up to a certain point, no great difficulty; but now, instead of being able to regard it as a special form of disease, we must allow that it is only one of a large class. We can understand the idea involved in the explanation of its symptoms resulting from a "diphtheritic process" excited by the presence of certain forms of bacteria. We can see the importance of physiological experiments like those of Panum, which had for their object the determination of the pathological effects of emboli of different substances upon the tissues they happened to be arrested in. The local changes which followed the introduction of a globule of mercury, corpuscles of healthy pus, or decomposing fluids, were found to differ to such a degree, that we must admit the value of Panum's conclusions, that the constitutional symptoms which occur in the last instance can only be explained on the supposition of a putrid intoxication, as he terms it, of the blood.

We begin to wish to know more of the minute organisms which seem to be the chief cause of the symptoms. We find our attention being attracted to the interesting question of their generation, their various forms, and their specific characters; and we feel disposed to examine the early researches of Ehrenberg, and the later attempts of Hallier, Cohn, Lister, Billroth, and others, to reduce them into some order of classification. Such a study is, however, the work of a lifetime. As pathologists, considering the important connection between the presence of bacteria, without special regard to their species, we desire to know why an embolus of pus or fibrin may excite slight, if any, pathological changes, unless it contains micrococci, or gives rise to them by decomposition; and then we find ourselves engaged in the difficult problem of the earliest changes produced in the process of inflammation.

We begin to perceive that we are crossing the line that separates the practical from the scientific, if such a line can be said to exist; for I venture to express the opinion that you will agree with me in think-

ing true practical knowledge is the result of scientific conception, and that all knowledge which is not practical in no way deserves the title of scientific. I have had at command, in the preparation of these lectures, numerous works of reference, as well as opportunity of testing the truth of the theory under discussion. The difficulty has been to judge of the merits of those works rather than to analyse their contents. To sift them thoroughly, and to do them justice, and this, too, with patience and good feeling, I need hardly assure you, was a task which might have occupied a very long period of investigation.



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